Did *Miranda* Diminish Police Effectiveness?

John J. Donohue III*

INTRODUCTION

The question of whether the Supreme Court's 1966 *Miranda*[1] decision has significantly hampered law enforcement is fascinating from both a substantive and a methodological perspective. On the substantive front, Professors Paul Cassell and Richard Fowles observe that Justice John Harlan's *Miranda* dissent insightfully warned that the majority decision could hamper law enforcement in a way that imposed large social costs.[2] How large these costs might be, Harlan admonished, "only time can tell."[3] In a remarkable series of papers, Professors Paul Cassell and Steve Schulhofer debate the effect of *Miranda*, with Cassell articulating the case for the prosecution, both literally and figuratively, and Schulhofer mounting a spirited defense.[4]

As part of this exchange, Cassell published a simple graph of national arrest rates for violent and property crimes, which showed a sharp reduction in

---

* Professor of Law and John A. Wilson Distinguished Faculty Scholar, Stanford Law School. I would like to thank George Fisher, Yale Kamisar, Steve Levitt, Peter Siegelman, Steve Schulhofer, and Charles Weisselberg for their extremely helpful comments. I also thank Paul Cassell and Richard Fowles for generously supplying their data and Connie Taylor for truly outstanding research assistance.

1. Miranda v. Arizona, 384 U.S. 436 (1966).

2. *See* Paul G. Cassell & Richard Fowles, *Handcuffing the Cops? A Thirty-Year Perspective on* Miranda*'s Harmful Effects on Law Enforcement*, 50 STAN. L. REV. 1055 (1998).

3. *Miranda*, 384 U.S. at 504 (Harlan, J., dissenting).

4. *See* Paul G. Cassell, *All Benefits, No Costs: The Grand Illusion of* Miranda*'s Defenders*, 90 NW. U. L. REV. 1084 (1996) [hereinafter Cassell, *All Benefits*]; Paul G. Cassell, *The Costs of the* Miranda *Mandate: A Lesson in the Dangers of Inflexible, "Prophylactic" Supreme Court Inventions*, 28 ARIZ. ST. L.J. 299 (1996); Paul G. Cassell, Miranda*'s Social Costs: An Empirical Reassessment*, 90 NW. U. L. REV. 387 (1996) [hereinafter Cassell, Miranda*'s Social Costs*]; Stephen J. Schulhofer, Miranda *and Clearance Rates*, 91 NW. U. L. REV. 278 (1996) [hereinafter Schulhofer, Miranda *and Clearance Rates*]; Stephen J. Schulhofer, Miranda*'s Practical Effect: Substantial Benefits and Vanishingly Small Social Costs*, 90 NW. U. L. REV. 500 (1996) [hereinafter Schulhofer, Miranda*'s Practical Effect*]. Many other scholars have explored this issue as well. *See, e.g.*, JOSEPH D. GRANO, CONFESSIONS, TRUTH, AND THE LAW (1993) (arguing that *Miranda* was an incorrect decision for both policy and constitutional reasons); YALE KAMISAR, POLICE INTERROGATIONS AND CONFESSIONS: ESSAYS IN LAW AND POLICY (1980) (discussing the Supreme Court's landmark police interrogation cases in the decade following *Miranda*); Richard A. Leo, *Inside the Interrogation Room*, 86 J. CRIM. L. & CRIMINOLOGY 266 (1996) (providing the results of the author's empirical study of police interrogation practices).

the proportion of violent crime cases that were reported to have been cleared (or solved) by the police.[5] Moreover, this deterioration in clearance rates in a number of crime categories appeared to begin in 1966, the year of the *Miranda* decision. This graphical evidence, coupled with work by Cassell and others, convinced Cassell that *Miranda* has seriously undermined the ability of police to fight crime.[6] Schulhofer was quick to point out that it is impossible to identify the cause of a change in a time series when there has been no effort to control for important systemic forces operating in the same direction as the change.[7] Specifically, Schulhofer noted that clearance rates fell sharply in 1966 at about the same time that crime rates started to soar. With police resources failing to keep up with the massive increase in crime, Schulhofer admonished, it is not surprising that clearance rates fell at that point.[8]

In response to Schulhofer's careful and probing examination of various competing explanations for the 1966 fall in clearance rates, Cassell has now teamed up with Professor Richard Fowles to test whether a multiple regression analysis of the time series of Federal Bureau of Investigation ("FBI") reported national clearance rates supports Schulhofer's proffered explanations.[9] On the basis of this statistical analysis, Cassell and Fowles reaffirm the view that *Miranda* caused clearance rates to drop sharply for certain crimes during the years 1966-1968.[10]

The repeated exchanges between Professors Cassell and Schulhofer on the impact of *Miranda* have generated many interesting insights and identified a vast array of complexities. To their credit, Cassell and Fowles have labored diligently in their present study to address these concerns. Although I had not closely followed the lively, contentious, and voluminous literature that has

---

5. *See* Cassell, *All Benefits*, *supra* note 4, at 1090; *see also* Cassell, Miranda*'s Social Costs*, *supra* note 4, at 439-40 (using arrest rates to argue that *Miranda* produces a loss in the number of cases solved). For decades, the Federal Bureau of Investigation has published crime data, including clearance rates, for whatever cities happened to report such information. *See* FEDERAL BUREAU OF INVESTIGATION, U.S. DEP'T OF JUSTICE, UNIFORM CRIME REPORTS, CRIME IN THE UNITED STATES [hereinafter UCR-[year]]. The *UCR* describe a case as "cleared" when the police have arrested the perpetrator or otherwise have solved the case. *See* note 46 *infra* and accompanying text. "Cleared" does not imply "convicted" or even prosecuted. For example, the murders of Nicole Brown Simpson and Ron Goldman were cleared as soon as O.J. Simpson was arrested. *See* notes 46-48 *infra* and accompanying text.

6. *See* Cassell, Miranda*'s Social Costs*, *supra* note 4, at 440 (claiming that 28,000 arrests for violent crimes and 79,000 arrests for property crimes were lost as a result of *Miranda*). Cassell interprets his own and other work as showing that the implementation of *Miranda* has reduced the number of confessions by criminals, but others have disputed this interpretation.

7. *See* Schulhofer, Miranda *and Clearance Rates*, *supra* note 4, at 280-89; Schulhofer, Miranda*'s Practical Effect*, *supra* note 4, at 510-15.

8. *See* Schulhofer, Miranda *and Clearance Rates*, *supra* note 4, at 288 (noting that in 1955 there were 121 police officers for every 100 reported violent offenses, but that this ratio fell to 45 officers in 1970 and 28 officers in 1996).

9. *See* Cassell & Fowles, *supra* note 2. Beginning in 1980, in addition to publishing clearance rates for cities, the FBI started publishing clearance rates for "total agencies"—agencies in cities as well as suburban and rural counties. *See* UCR-1980, *supra* note 5, at 182 tbl.20. Cassell and Fowles use the clearance rates for cities rather than for "total agencies" since no data in this latter category is reported prior to 1980.

10. *See* Cassell & Fowles, *supra* note 2, at 1118.

tried to examine the impact of *Miranda*, the *Stanford Law Review* has asked me for my thoughts on the statistical methodology that Cassell and Fowles have employed.[11] While the tight time constraint does not permit me to address all of the relevant empirical issues, I will emphasize a few major points. First, I will offer some general thoughts on the enterprise of determining the impact of a judicial decision and some specific comments on my a priori views concerning the impact of *Miranda*. Second, since any statistical study is only as good as its data, I will discuss the limits of the clearance rate data upon which the Cassell-Fowles study is based. Third, I will comment on the limits and difficulties of the particular statistical methodology of this study—the interrupted time-series regression. Fourth, I will explore the robustness of the study's regression results when presented with changes in specification and functional form,[12] and then I will offer some concluding remarks.

I. SOME THOUGHTS ON *MIRANDA* AND EVALUATING THE IMPACT OF A JUDICIAL DECISION

As someone who has spent a great deal of time investigating and studying the impact of various legal interventions, I begin with the assumption that, in general, it is hard to find *any* direct effect of a legal intervention and that when impacts are found they tend to be rather small. The complex forces that shape major social phenomena do not tend to shift dramatically or quickly in response to a legal intervention. Thus, if one looks at the degree of desegregation of southern schools in the wake of the momentous decision in *Brown v. Board of Education*,[13] one sees almost no change in the percentage of black students attending all-black schools for well over a decade.[14] Similarly, if one looks at birth rates before and after *Roe v. Wade*,[15] it is surprisingly difficult to detect any impact of that important case, despite the substantial subsequent increase in the documented number of legal abortions.[16] The point is not that legal interventions don't matter. Indeed, I believe that the impact certainly of *Brown* and possibly of *Roe* has been enormous. But if one seeks to quantify

---

11. I thank Paul Cassell and Richard Fowles for supplying their data to me, without which I could not have furnished this comment in the abbreviated time period that I was allotted.

12. Cassell and Fowles conclude on the basis of their regression analysis that a post-mid-1966 time dummy, which they argue captures the effect of *Miranda*, consistently shows that clearance rates for violent crimes fell sharply beginning in mid-1966. This conclusion would be said to be "fragile" if slight changes in the regression equation would eliminate the statistical significance of the estimated coefficient or perhaps even reverse its sign from negative to positive. Conversely, if the sign, magnitude, and statistical significance of the post-1966 dummy are largely unaffected by changes in the regression equation, then one would conclude that the finding was "robust."

13. 347 U.S. 483 (1954).

14. *See* John J. Donohue III & James Heckman, *Continuous Versus Episodic Change: The Impact of Civil Rights Policy on the Economic Status of Blacks*, 29 J. ECON. LITERATURE. 1603 (1991). Border states desegregated relatively quickly following *Brown*, but the number of blacks in these states was so small that the aggregate desegregation figures for black students were little changed. *See id.* at 1627 & n.31.

15. 410 U.S. 113 (1973).

16. *See* PHILIP B. LEVINE, DOUGLAS STAIGNER, THOMAS J. KANE & DAVID J. ZIMMERMAN, ROE V. WADE AND AMERICAN FERTILITY (National Bureau of Economic Research, Working Paper No. 5615, 1996).

this impact through seemingly plausible statistical measures, one frequently finds that the measurable consequences of Supreme Court commands, when identifiable, tend to be somewhat glacial.[17]

None of this is to suggest that *Miranda* could not have had a pronounced impact, but there is a large enough body of evidence examining a vast array of legal interventions to justify an a priori assumption that the *observed* impact would tend to be small. Moreover, some specific elements of criminal justice might buttress this a priori view concerning *Miranda*. First, the police did not like *Miranda*, and therefore one might expect them to take steps to minimize its influence.[18] Second, the typical individual who could benefit from a *Miranda* warning is often not fully aware that silence is the best policy, whether because of lack of sophistication, lack of intelligence, or the stress induced by any custodial interrogation. Studies seem to suggest that most individuals hearing the typically rote incantation of the *Miranda* warning do not invoke their protection.[19] Third, as I will discuss in greater detail below, even if *Miranda* did harm law enforcement, this effect might not necessarily be expected to influence measured clearance rates.[20]

On the other hand, *Miranda* might be one of the few Supreme Court decisions that could change behavior quickly: It commands action by police officers rather than the general public, and one would expect that this specialized audience would rapidly become acquainted with the dictates of the decision. Moreover, if one can assume that the police were fighting crime efficiently to begin with—perhaps a strong assumption given the customary performance of monopolies and public bureaucracies—then one would expect that the *Miranda* decision would tend to hamper their efforts to at least some degree. Nonetheless, the *observed* effect of *Miranda* on measures of police performance would likely be small: If the police truly had a much harder time in fighting crime, they would adopt alternative measures to address the situation. As recent experience in New York City suggests, large increases in police

---

17. Indeed, Gerald Rosenberg has built a theory about the alleged fecklessness of the Supreme Court in generating major social change, which, in essence, asserts that the internal dynamics of long-lived social institutions are difficult to dislodge abruptly. *See* GERALD N. ROSENBERG, THE HOLLOW HOPE: CAN COURTS BRING ABOUT SOCIAL CHANGE? (1991).

18. While running for President in 1968, candidate Richard Nixon expressed the view of many police officers that *Miranda* and other Warren Court decisions "had the effect of seriously hamstringing the peace forces in our society." Yale Kamisar, *How to Use, Abuse—And Fight Back with—Crime Statistics*, 25 OKLA. L. REV. 239, 241 (1972) [hereinafter Kamisar, *Crime Statistics*]. According to Kamisar, "Police and prosecutors strenuously resist what they like to call 'tighter restrictions' on their powers." Yale Kamisar, *On the Tactics of Police-Prosecution Oriented Critics of the Courts*, 49 CORNELL L.Q. 436, 440 (1964) [hereinafter Kamisar, *Tactics*]. Richard Leo has concluded that, "although the requirement of warnings undoubtedly causes some suspects to avoid cooperating with their interrogators, police have successfully adapted their practices to the legal requirements of *Miranda* by using conditioning, deemphasizing, and persuasive strategies to orchestrate consent to custodial questioning in most cases." Richard A. Leo, *The Impact of* Miranda *Revisited*, 86 J. CRIM. L. & CRIMINOLOGY 621, 675 (1996).

19. A 1994 study of police interrogation in Salt Lake County found that 12.1% of suspects who were questioned "invoked their rights before police were successful in interrogation." Paul G. Cassell & Bret S. Hayman, *Police Interrogation in the 1990s: An Empirical Study of the Effects of* Miranda, 43 UCLA L. REV. 839, 860 (1996). In a separate study, Richard A. Leo found that 21.7% of suspects invoked their *Miranda* rights. *See* Leo, *supra* note 4, at 286 (1996).

20. *See* text accompanying notes 47-51, *infra*.

forces appear to generate large reductions in crime.[21] This implies that the real cost of *Miranda* likely is a resource cost. Presumably, the police could fully offset any *Miranda*-induced increase in crime by implementing alternative crime-fighting measures. If, for example, a 5% increase in police costing about $4 billion a year would reverse the crime increase, then *Miranda* annually costs $4 billion.[22]

This analysis illustrates the misleading nature of Justice White's unnecessarily inflammatory comment in his *Miranda* dissent that in "some unknown number of cases the Court's rule will return a killer, a rapist or other criminal to the streets and to the environment which produced him, to repeat his crime whenever it pleases him."[23] Of course, the final phrase is overblown—whatever *Miranda*'s defects, it did not provide an unconditional pardon for all future crimes or repeal the criminal laws. But more centrally, in some unknown number of cases, killers go free because taxpayers don't hire more police to track them down. While there is, doubtless, an added psychic toll when an identified criminal is freed by a legal impediment to prosecution, any untoward effects of *Miranda* on *crime* can be offset through the implementation of other, albeit costly, measures. Indeed, Cassell and Fowles conclude on the basis of their regression findings that, while *Miranda* lowered clearance rates overall, it did not lower clearance rates in murder cases.[24] This finding is consistent with the view that, at least for the most serious crime, any necessary compensating anticrime measures have been implemented.[25] Of course, the finding is also consistent with the view that *Miranda* simply had no effect on murder clearances in the first place.

II. DATA

The heart of the Cassell-Fowles study is the apparent sustained drop in the FBI-reported clearance rates for violent and property crime beginning during the period from 1966-1968, which the study attributes to the June 1966 *Miranda* decision.[26] There are two basic ways to challenge this conclusion. The first is to question whether there was any deviation from the prior trend in the clearance rate that occurred after June 1966. If such post-1966 drop in clearance rates did occur, the second basis of attack then questions whether any such change should be attributed to *Miranda*. Since FBI crime data are notoriously bad, one must ask whether data problems infect the Cassell-Fowles findings on either of these two grounds.

---

21. Between 1993 and 1996, murders in New York City dropped by 49%. David Whitman, *On Not Believing the Good News*, U.S. NEWS & WORLD REP., Dec. 29, 1997, at 44. Over this same period the police force rose from about 29,000 officers to about 37,000 officers. *See* UCR-1993, *supra* note 5, at 330 tbl.78; UCR-1996, *supra* note 5, at 335 tbl.78.

22. The text abstracts from consideration of the deadweight losses associated with the increased taxation and from the possibility that society is unwilling to spend the added resources needed to reduce crime because the marginal cost of crime is lower than the marginal cost of crime reduction.

23. *Miranda*, 384 U.S. 436, 542 (1966) (White, J., dissenting).

24. *See* Cassell & Fowles, *supra* note 2, at 28, 1086 tbl.II.

25. Cassell and Fowles' finding that *Miranda* had no impact on the murder clearance rate undermines Justice White's notion that murderers would be set free because of *Miranda*.

26. *See* Cassell & Fowles, *supra* note 2, at 1086 tbl.II & 1088 tbl.III.

A. *Have Clearance Rates Really Fallen?*

First, there is reason for concern about whether the clearance rate—the ratio of solved to total cases—is accurately measured. We know that there has been political manipulation of crime data and that over time much more crime is being reported to the FBI.[27] Both of these facts would cause measured clearance rates to fall as data quality improved. Were the high clearance rates of the 1950s and early 1960s that appear in the FBI data the result of cooking the books to make the police look better? If so, why did they stop fudging the crime data in the late 1960s? We know that police practices in general were drawing greater scrutiny in the late 1960s, which in turn would presumably reduce the degree of data manipulation.[28] Or perhaps anger over *Miranda* suddenly made police departments more willing to reveal a lower clearance rate, since there was an easy scapegoat for the relatively poorer performance.[29]

Even apart from any conscious manipulation of FBI crime data, police departments have gotten better at recording, computerizing, and reporting crime information. Consequently, the FBI crime statistics have swelled from their unrealistically low initial levels. Ironically, as FBI crime statistics have improved, they have given the inaccurate picture that crime has risen during periods when the more accurate National Crime Victimization Survey ("NCVS") indicates that crime stayed level or even fell.[30] Of course, this may have potentially serious implications for the Cassell-Fowles study under the

---

27. In 1966, Police Commissioner Howard Leary of New York reported that better recording of crime resulting from the initiation of a central reporting unit caused reported robberies to jump 164% and reported burglaries to jump 139% in one year, when he estimated the actual increase in crime to be less than 10%. *See* Kamisar, *Crime Statistics*, *supra* note 18, at 243. Chicago experienced the same phenomenon in the early 1960s as it abandoned the practice of ignoring reports of crime "to save work and make the district look better on paper." *Id.* This effect was further exacerbated when a publicity campaign to encourage the public to report crime by calling a central number facilitated easier and more complete reporting of crime. *See id.*; *see also* notes 31 & 39, *infra* (discussing problems in clearance rate data).

28. *See* Stephen J. Schulhofer, *Bashing* Miranda *Is Unjustified—And Harmful*, 20 HARV. J.L. & PUB. POL'Y 347, 369 (1997).

29. As Yale Kamisar observed over 25 years ago:

> As a police department grows aware that it is generally regarded as understaffed, overworked and judicially "hamstrung," it becomes less inhibited about producing fuller and more accurate crime statistics. An undermanned force, which is "handcuffed" by the courts to boot, can hardly be held responsible for sharp increases in crime.

Kamisar, *Crime Statistics*, *supra* note 18, at 244.

30. Beginning in 1973—alas, too late for a before-after assessment of the impact of *Miranda*—far better data on the true incidence of crime than that reported by the FBI has been available from the NCVS. This annually collected data series, which records incidents of criminal victimization regardless of whether they have been reported to police, shows crime falling or remaining relatively stagnant for the period from 1973-1993 in the categories of rape, aggravated assault, burglary, and robbery. Yet if one looks at the FBI reports for the same period, the pattern is generally relentlessly upward for all crimes except burglary. In each case, the regression of the NCVS crime figures on a time trend reveals a significantly negative trend from 1973-1993. With the exception of burglary, the same regressions on the FBI statistics show a significantly positive trend over the same period. Perhaps as much as one-quarter of the upward trend in crime over the last twenty years indicated by the FBI statistics reflects an upward trend in the number of cases reported to police. *See* SCOTT BOGGESS & JOHN BOUND, DID CRIMINAL ACTIVITY INCREASE DURING THE 1980S? (National Bureau of Economic Research, Working Paper No. 4431, 1993).

following circumstance: To the extent that the early underreporting of crime tended to ignore the less important or unsolved cases, then reported clearance rates would tend to be exaggerated. Indeed, a careful sociological study of a California police department conducted *before* the *Miranda* decision—and therefore obviously not designed to explain away the subsequent clearance rate drop—describes exactly this problem of underreporting of unsolved cases.[31] Over time, as more data was recorded and reported, FBI crime statistics would swell and clearance rates would fall, since, by hypothesis, the previous underreporting depressed the denominator of the clearance rate more than the numerator.[32]

One check on these possible data problems is to limit one's analysis to the case of murder, which is the most accurately reported of all crimes in the FBI data.[33] On their face, the murder clearance rate figures reported by the FBI for the 1950s seem unrealistically high. Prior to 1966, the FBI reports that clearance rates for murder and nonnegligent homicide were always in excess of 90%.[34] Indeed, for 33 cities with population over 250,000, with a combined population of 22.4 million, the murder clearance rate figure for 1951 is given as 97.1%.[35] A rough estimate of the murder rate in these cities in 1951 is 6.62 per 100,000, so that the estimated number of murders would be 1483.[36] The listed clearance rate would suggest that the police solved 1440 of these 1483 murders. While many homicides—particularly intrafamily homicides—are easy to solve, I am very dubious that big-city police could ever achieve such an impeccable clearance record, even during the simpler, less drug- and

---

31. The study reports that many offenses were recorded as "suspicious circumstances" instead of as reported offenses. This recording practice tends to exaggerate clearance rates. Indeed, one detective expressed resentment at a rule in his department requiring all complaints to be recorded as actual offenses because the resulting lower clearance rates made the police look bad:

> Well, we're an honest police department. All these other departments that have these fancy clearance rates—we know damned well they're stacking the cards. It's easy to show a low crime rate when you have a category like suspicious circumstance to use as a wastebasket. Here, at least we know what's going on—everything is reported.

JEROME SKOLNICK, JUSTICE WITHOUT TRIAL 173 (1966). Thus, any ensuing increase in the accuracy of crime figures would depress clearance rates. To get a sense of the magnitude of this effect, consider Skolnick's claim that in one department roughly 20-25% of burglary complaints were listed only as suspected offenses. *See id.* at 172. Thus, given the national burglary clearance rate of 28% in 1962, *see id.* at 173, an increase in recording of these complaints as actual offenses could swell reported crime by, say, 10%, thereby causing the clearance rate to drop to 25.5%—a drop of 10% in the clearance rate.

32. Conversely, if the underreporting were random, then the fact that more crime is now being reported in FBI crime statistics should not influence the clearance rate, since cases cleared and total crime would simply be rising together at the same rate.

33. David Cantor & Lawrence E. Cohen, *Comparing Measures of Homicide Trends Methodological and Substantive Differences in the Vital Statistics and Uniform Crime Report Time Series*, 9 SOC. SCI. RES. 121 (1980).

34. *See* Cassell & Fowles, *supra* note 2, at 1084 fig.3.

35. *See* UCR-1952, *supra* note 5, at 47.

36. One not atypical anomaly with the FBI data is that we know the clearance rates for 33 cities with populations over 250,000 (with a total population of 22.4 million), but we are only given the murder rate for a sample of 39 cities with populations over 250,000 (and a total population of 26.6 million). For this larger sample, the murder rate in 1951 was 6.62/100,000 (based on 1765 known murders). *See* UCR-1951, *supra* note 5, at 74.

gang-filled world of the 1950s.[37] Presumably, the almost entirely white police forces would not have been highly concerned with murders in racial ghettos, and the admittedly smaller number of stranger murders would have been difficult to resolve—even in the more freewheeling days before the Warren Court decisions on criminal procedure.

My hunch is that when there was little scrutiny over their actions, the police tended to "close" cases in a highly self-serving way.[38] When police practices came under greater scrutiny, as reflected in the criminal procedure docket of the United States Supreme Court in the mid-1960s, police departments professionalized and crime reporting became standardized, resulting in more accurate, albeit less flattering, records of police efficacy.[39] Another vehicle for this enhanced scrutiny was the Supreme Court's 1963 decision in *Gideon v. Wainwright*,[40] which launched an amazing transformation in criminal justice as all criminal defendants suddenly had a right to court-appointed counsel, thereby increasing the monitoring of police conduct and possibly lowering *actual* clearance rates as well.[41]

---

37. Note also that the composition of crime can affect clearance rates strongly. Presumably, reported domestic violence crime is "cleared" at a much higher rate than violent crime committed by drug gangs. Conceivably, the sharp jump in the use of illegal drugs in the second half of the 1960s contributed to the deterioration in clearance rates. Ideally, one would like to include some measure of the illegal drug market as an explanatory variable, but the available data on this issue is often highly unreliable. *See* Jeff Leen, *Number Jumble Clouds Judgment of Drug War*, WASH. POST, Jan. 2, 1998, at A1.

38. Archival research into county records of criminal prosecutions might reveal the correspondence, or lack thereof, between the number of cases that were recorded as "cleared" and the number that led to indictments. My working hypothesis would be that the ratio of indictments to clearances was far lower in the 1950s than it is today, which would suggest that the published clearance rates of the 1950s were exaggerated.

39. *See* Schulhofer, *supra* note 28, at 368-69; *see also* notes 27 & 31, *supra* (noting that more accurate reporting of crimes can make clearance rate figures look worse). Skolnick reports a pre-*Miranda* case in which the police interrogated a defendant named James who provided the police with more than 400 burglary clearances: "I witnessed several interrogations of James regarding burglaries he had presumably committed and, in my opinion, it was relatively simple for him to 'fake' clearances. One need not have been exceptionally shrewd—and James was—to sense the detectives' pleasure at writing off old cases." SKOLNICK, *supra* 31, at 178. Indeed, in part for this cooperation, James only served 30 days in jail after being prosecuted for only one crime. *See id.* Skolnick quotes a detective who was dismayed at the consequence of having "this clearance business hanging over our heads." *Id.* at 179. The detective continued:

> We get guys like this and they hand us clearance after clearance and on FBI books we look terrific. But the fact is that large numbers of burglaries are committed by a relatively small group and when we get one of them we have to give him a good break in order to make ourselves look good. It's a ridiculous system, but that's the way they run things upstairs.

Id. at 179.

40. 372 U.S. 335 (1963).

41. Providing legal representation to all criminal defendants could influence clearance rates in a number of ways, even if the appointment of the lawyer came after an arrest, and, hence, after the initial clearance. First, since many criminal defendants are repeat players, a lawyer's advice not to make statements to the police could influence the defendant's willingness to speak upon subsequent arrest. Following *Gideon*, far more criminals were likely to hear this advice from their own attorney, and over time this might have curtailed custodial statements by defendants even absent the *Miranda* decision. Second, the previously mentioned practice of securing confessions to an array of unresolved crimes might be hampered if lawyers prevent their clients from confessing to crimes other than the ones for which they are currently charged. Third, the knowledge on the part of police that criminal defendants would soon be telling their attorneys of any police misconduct would presuma-

These uncertainties about the accuracy of the data might counsel giving greater weight to the analysis of the presumably more accurate murder clearance rates, as opposed to clearance rates for other crimes. This approach would be unfavorable to the general Cassell-Fowles position, since, as previously noted, Cassell and Fowles report that they find no significant effect of *Miranda* on homicide clearance rates.[42] Murder clearance rates showed a rather smooth decline from the late 1950s through the 1970s, with no evidence of a break in the series around 1966 that might be attributed to *Miranda*.[43] Thus, we are left with the fact that the crime data are likely to be inaccurate in ways that would buttress the Cassell-Fowles hypothesis, and, when we look at the most accurate of the still-flawed data, no evidence of a *Miranda* effect can be found on murder clearance rates.[44] This point, while perhaps decisive, is itself subject to criticism on the following ground: If *Miranda* did hamper police effectiveness, the negative consequences could have been masked as police resources were reallocated to homicide cases. Thus, the *Miranda* effect could be large, even if it doesn't show up at all in the crude clearance numbers for murder; this hypothesis is consistent with the Cassell-Fowles finding that the clearance rates for lesser crimes did fall.[45]

B. *Is* Miranda *the Reason for Any Fall in Clearance Rates?*

So far we have probed whether post-1966 clearance rates have truly deviated from their previous trend. But, even if they have, there is another problem with the accuracy of the data that might undermine *Miranda* as a possible explanation for the decline. The FBI defines clearance as follows:

> For UCR purposes, law enforcement agencies clear or solve an offense when at least one person is arrested, charged with the commission of the offense, and turned over to the court for prosecution. Clearances recorded in 1966 may be for offenses which occurred in prior years. Several crimes may be cleared by the arrest of one person, or the arrest of many persons may clear only one offense. Law enforcement agencies may clear a crime by exceptional means when some element beyond law enforcement control precludes the placing of formal charges

---

bly discourage some coercive conduct that might have led to clearances in the official records (whether or not the coerced confessions were accurate).

42. *See* Cassell & Fowles, *supra* note 2, at 1086 tbl.II.

43. *See id.* at 1084 fig.3.

44. In their rejoinder to this response, Cassell and Fowles note that motor vehicle theft clearance rates did experience a drop at about the time of the *Miranda* decision and that motor vehicle theft is probably the second most accurately reported crime. *See* Paul G. Cassell & Richard Fowles, *Falling Clearance Rates After* Miranda*: Coincidence or Consequence?*, 50 STAN L. REV. 1181 (1998). Despite their relative accuracy, though, the auto theft figures are still not as reliable as one might like. For example, in 1991, when the *UCR* reported 1.7 million auto thefts, *see* UCR-1991, *supra* note 5, at 49, the more reliable NCVS reported 2.1 million thefts from households (not including thefts from business and governmental entities that would be captured in the *UCR*), *see* BUREAU OF JUSTICE STATISTICS, U.S. DEP'T OF JUSTICE, *Criminal Victimization in the United States* 16 tbl.1 (1991). In other words, at least 400,000 auto thefts never made it into the FBI data in that year, reflecting an error rate in the neighborhood of 20-25%.

45. *See* Cassell & Fowles, *supra* note 2, 1086 tbl.II & 1087 tbl.III. The hypothesis that police shifted resources to homicide cases to maintain their clearance rates could presumably be tested: One could determine whether the number of officers and resources assigned to the homicide divisions of police departments has been growing relative to all officers and police resources.

> against the offender. Examples of circumstances allowing such clearances are the death of the offender (suicide, justifiably killed by police or private citizen, etc.); the victim's refusal to cooperate with prosecution after the offender has been identified; or the denial of extradition because the offender committed another crime in a different jurisdiction and is being prosecuted there. In all exceptional clearance cases, law enforcement must have identified the offender, have enough evidence to support arrest, and know the offender's location.[46]

As this passage shows, the definition of a "cleared case" is not entirely straightforward. To explore how police fill out the FBI clearance rate data, my research assistant called ten police departments across the country and asked each when it considers a case to be "cleared."[47] The overwhelming response from this admittedly small and nonrandom sample was that as soon as an arrest was made, regardless of whether the District Attorney was willing to prosecute the individual, the police considered the case cleared and so reported the case to the FBI.[48] In the vast bulk of cases, then, *Miranda* would have no impact on clearance rates because *Miranda* regulates only custodial interrogations, which typically occur *after* the arrest has been made.[49] Since *Miranda* comes into play only after the individual has been arrested, and police consider a case cleared as soon as an arrest is made, there is only one situation in which *Miranda* could have any bite: when a suspect who has been arrested for crime A would confess to crimes B and C during an interrogation about crime A, but because of *Miranda*, stops talking and doesn't confess to crimes B and C or fails to implicate another for such crimes.[50] In either event, crime A is "cleared" according to current police practice. Apart from this "other-crimes" effect, there is little scope for *Miranda* to influence clearance rates.[51]

This analysis does not absolve *Miranda* of the charge that it has a negative impact on crime fighting. If *Miranda* significantly stops the flow of damaging statements by criminals to the police, it could well reduce the rate of successful prosecutions of crime even if it has no impact on measured clearance rates. To the extent my brief exploration into current police practice is widely representative of police practice in the late 1960s and the "other-crimes" effect is small, however, the conclusion of the Cassell-Fowles paper concerning the effect of *Miranda* on clearance rates is seriously compromised.[52]

---

46. UCR-1996, *supra* note 5, at 203.

47. The ten cities called were: Bakersfield, Cal.; Beaumont, Tex.; Cincinnati, Ohio; Denver, Colo.; Fresno, Cal.; Lubbock, Tex.; Mesa, Ariz.; Oakland, Cal.; Phoenix, Ariz.; and Reno, Nev.

48. Every police officer contacted in the ten cities, except for one in Phoenix, indicated that an arrest was sufficient to report a crime as cleared.

49. *See* Miranda v. Arizona, 384 U.S. 436, 467 (1966).

50. If police frequently detained suspects to interrogate them in order to secure information about whether the suspect should be arrested, then *Miranda* could discourage inculpatory remarks prior to arrest. Our discussions with police in ten cities suggested, however, that the number of cases that fell into this category was exceedingly small.

51. Furthermore, it is difficult to attribute any such "other-crimes" effect to *Miranda* rather than to other possible causes such as *Gideon*. *See* notes 40-41 *supra* and accompanying text.

52. The "other-crimes" effect may not be uniform across crimes, however. Such variation might explain why Cassell and Fowles find no *Miranda* effect for certain crimes (e.g., murder, rape,

III. METHODOLOGY

When trying to ascertain the effect of a law through statistical means, most researchers hope to find a "natural experiment" in which the law applies in one jurisdiction but not in another. For example, a researcher trying to assess the impact of an increase in the minimum wage would be far more optimistic about the enterprise if certain states raised their minimum wage while other states did not. It would be powerful evidence if many states raised their minimum wages at different times and then experienced drops in employment, while the states that did not change their minimum wage saw no decline in employment. The so-called "panel data analysis," which would use the variation both across states and over time would allow a more accurate causal assessment than would be possible if one attempted to analyze the impact of a federally mandated increase in the minimum wage that applied to all workers at the same time. In the latter case, known as an interrupted time-series analysis, the researcher would only be able to compare events before and after the relevant legal change. Deriving valid statistical conclusions is difficult in this situation, because many factors besides the law that the statistical model cannot capture may be changing at the same time.

Thus, Cassell and Fowles begin at somewhat of a disadvantage since they base their study on an interrupted time-series analysis stemming from a single federally imposed mandate, rather than on the more desirable type of panel data analysis, which examines the experience of different states over time. Conceivably, one might find some cross-sectional variation by looking at those states that imposed limitations on custodial interrogations prior to the *Miranda* decision; this might be another avenue of research to test the results of their study.[53]

Although the interrupted time-series approach may be the only available mode of analysis of a federally imposed mandate, there have been enough disastrous applications of this approach in analyzing legal interventions to

---

and aggravated assault) and seem to see large effects for other crimes (e.g., robbery, burglary, and vehicle theft) that are often part of a pattern or practice of criminal conduct. For example, the confessions of one active burglar may solve scores of burglaries. Note, though, that the lower clearance rates attributable to the "other-crimes" effect should have less effect on crime than would a lower clearance rate owing to the inability to identify an offender in the first place. If a criminal gets caught and sent to prison, yet fails to confess to some additional crimes because of *Miranda*, the bulk of any incapacitative or deterrence benefit may still be achieved. *See* note 41 *supra*. In addition, when the police need to clear other offenses, the police can always grant the suspect immunity and compel him to provide information about other crimes.

53. Alternatively, one might be able to make use of the fact that the FBI used a *Miranda*-type warning long before 1966. (The Supreme Court in *Miranda* alludes to the FBI's "exemplary record of effective law enforcement while advising any suspect or arrested person, at the outset of the interview, that he is not required to make a statement, [and] that any statement may be used against him . . . ." 384 U.S. 436, 484 (1966).) This implies that clearance rates for federal crimes should not have been affected by the *Miranda* decision even though they would be influenced by the social forces operating at that time. Consequently, if FBI clearance rates fell at about the time of the *Miranda* decision, then social forces—not *Miranda*—were the cause. Such a finding would weigh against the Cassell-Fowles thesis. Conversely, if the FBI experienced no fall in clearance rates after *Miranda*, the Cassell-Fowles thesis would be buttressed. Unfortunately, the FBI does not keep records of its clearance rates, so I was unable to perform this test.

make us wary of the results.[54] The instant case particularly warrants suspicion for two reasons. First, as Cassell and Fowles note, researchers have not successfully explained clearance rates, which is the main dependent variable in the Cassell-Fowles study.[55] A weak statistical model exacerbates the fragility of time-series analysis. If, at the outset, the model cannot explain variations in the variable being tested—clearance rates in the Cassell-Fowles study—identifying the impact of a specific legal change on the variable becomes much more difficult. Second, during the late 1960s American society underwent dramatic change across many fronts, which could well have dampened police clearance rates, but which are difficult to include as explanatory variables in regression models. A number of studies have foundered, consequently, when they wrongly attributed changing outcomes to a legal intervention without adequately controlling for these powerful demographic, political, cultural, and social shifts.[56]

Let me give one example of this latter problem—Sam Peltzman's work on the impact of auto safety regulation.[57] Peltzman concluded that federally mandated safety features made drivers feel safer, thereby inducing more reckless driving, which in turn caused the number of pedestrian deaths to increase sharply.[58] In essence, Peltzman compared the periods before and after the introduction of auto safety legislation and found that motor vehicle occupant death rates fell, while pedestrian death rates rose.[59] The economist Richard Nelson raises the appropriate concerns about Peltzman's work:

> There were a lot of things other than seat belts that were different about the post-1965 period. Compared with the earlier period, the post-1965 (until 1970) era was one of high employment and prosperity. The "off trend" rise in nonfatal reported accidents could well result partially from the rise in the fraction of new cars . . . . The rise in pedestrian fatalities, which Peltzman stresses, surely needs more complete analysis than he gives to the problem. Although part of the rise may have been due to more dangerous driving habits, some of it might be due to the nonlinear effect of more pedestrians, to more cars, or to their interaction. Demographically, the huge post-World War II birth cohort was coming of age, and one can doubt that the associated rise in youth culture, and the effects it might have had on auto safety, is adequately caught by Peltzman's coefficient on size of group. This was an era of drugs, alcohol, and youth rebellion, for reasons that economists' models may not grasp very well. Certainly Peltzman did not even try to grapple seriously with the reasons. There was the war in Vietnam and its domestic influences. Might this have been related to the phenomena in question? I don't know, but "deviation from trend" obviously cannot be ascribed

---

54. For example, there has been enormous criticism of the time-series analysis of the impact of the death penalty conducted by Isaac Ehrlich, *The Deterrent Effect of Capital Punishment: A Question of Life and Death*, 65 AM. ECON. REV. 397 (1975). Some of the criticisms are discussed in ROBERT COOTER & THOMAS ULEN, LAW AND ECONOMICS (2d ed. 1996); *see also* note 57 *infra* and accompanying text (discussing Sam Peltzmann's work on seatbelt regulation).

55. *See* Cassell & Fowles, *supra* note 2, at 1074.

56. *See* note 54 *supra*.

57. *See* Sam Peltzman, *The Regulation of Automobile Safety*, *in* AUTO SAFETY REGULATION: THE CURE OR THE PROBLEM? 1 (Henry G. Manne & Roger LeRoy Miller eds., 1976).

58. *See id.* at 17-19.

59. *See id.* at 19 tbl.3.

> to "safety legislation" until the effect of these other factors is carefully assessed.[60]

Nelson's critique devastated Peltzman's thesis, as all serious economists, econometricians, and statisticians now concede.[61]

Before one accepts Cassell and Fowles' conclusion that "deviation from trend" after 1965 can be attributed to *Miranda*, it is certainly worth considering whether their regression model adequately captures the array of events that changed both society and the criminal justice system in the mid-1960s. Any of these events could have affected clearance rates. It might not be surprising that a period of drugs, alcohol, youth rebellion, domestic resistance to war, and political/racial violence coupled with hostility to police would be associated with falling clearance rates regardless of what the Supreme Court was doing in the realm of criminal procedure. While one might think that if these factors explained the drop in clearance rates, the rates would have rebounded when at least some of these factors elapsed, this view is not always correct.[62] In any event, even though the era of youth rebellion subsided, the world of drugs, gangs, and crime are clearly more pernicious in the period after the mid-1960s than they were in the preceding fifteen years. Neither Cassell and Fowles, nor any other researchers, have found a way to control for these influences in regression models, so the Cassell-Fowles article implicitly attributes all of these effects to *Miranda*.

IV. SPECIFICATION

If one can get by the data problems and the need for reliance on an interrupted time-series analysis, then one must address the issue of the appropriate specification of the regression equation. Correct specification of a regression model is an extremely important and difficult exercise, which requires that the analyst use both the appropriate explanatory variables and the appropriate functional form.[63] In preparing my own regression model, I began with the

---

60. Richard R. Nelson, *Comments on Peltzman's Paper on Automobile Safety Regulation*, *in* AUTO SAFETY REGULATION, *supra* note 57, at 63, 65-66.

61. Note that had Peltzman included a "*MIRANDA*" dummy in his regressions, rather than an auto safety regulation dummy, he would have received a statistically significant coefficient as well. But we certainly wouldn't want to blame increased traffic accidents on *Miranda*.

62. The failure of the removal of a causal element to restore the status quo ante, which is called hysteresis or the problem of symmetric causation, is a major issue in economics and sociology today. *See* STANLEY LIEBERSON, MAKING IT COUNT: THE IMPROVEMENT OF SOCIAL RESEARCH AND THEORY 62-87 (1985). For example, removing the bullet that kills a young man does not bring him back to life.

63. The importance of functional form can be illustrated with the following example. With data on the areas of thirty circles, ranging from one to thirty in radius, we might try to use regression to estimate the relationship between area and radius. But if we simply regressed area on radius, rather than the theoretically appropriate regression of area on radius-squared, our estimated equation would be AREA = -519.4 + 97.4 * RADIUS. Even though this equation yields high t-statistics and an adjusted R-squared of .94, it is a terrible equation. For example, when the radius is small, this equation predicts a theoretically impossible negative area. With the proper specification of the functional form, however, we would obtain the correct equation: AREA = 3.14159 * RADIUS-SQUARED—the familiar formula that AREA = πr^2.

The lesson is obvious: Even when trying to estimate an equation where we know that the dependent variable is fully explained by a single perfectly measured explanatory variable, we can get

following question about the choice of the dependent variable: Should the regression model try to explain the clearance rate or the natural logarithm of the clearance rate?[64] The regression model is intended to estimate the effect of explanatory variables, such as the amount of crime or the number of police, on the dependent variable, which is some measure of clearance rates. If these relationships are estimated accurately and precisely, then we would conclude that the regression model fits the data well. One measure of how well the model explains the data is the R-squared value, which tells what proportion of variation in the dependent variable is explained by the regression. Because I obtained a slightly higher R-squared value using the natural log of the relevant clearance rate, I used that as the dependent variable in my regressions.[65]

A. *Explanatory Variables*

Ideally, to ascertain the effect of *Miranda* on clearance rates, we would like a regression equation that included every significant influence on national crime clearance rates as an explanatory variable. Increasing the number of police and police resources should lead to more clearances, so these are obvious explanatory variables.[66] Conversely, increased crime may overwhelm the ability of police to process complaints and resolve cases, thereby lowering clearance rates, so some measure of crime should be included in the regression model. Indeed, as Schulhofer noted in earlier work, it may not just be the levels of crime and police resources that are important in explaining clearance

---

very disappointing regression results if the wrong functional form is employed. Since the clearance rate regressions of this study use very imperfect data and since we don't know and/or can't quantify all of the relevant explanatory variables, and we are uncertain about the proper functional form of the equation, we must entertain the prospect that the estimated clearance rate equation is performing badly.

64. Imagine that adding an additional 10,000 police officers would be expected to increase the violent crime clearance rate by 2%, regardless of the current total number of police or the current clearance rate. That is, adding 10,000 police officers would cause an initial clearance rate of 20% to rise to 22% or an initial clearance rate of 40% to rise to 42%. This means that the relationship between the number of police officers and the clearance rate is linear, and the appropriate dependent variable would simply be the actual clearance rate. On the other hand, the effect of adding 10,000 police officers might generate a fixed *proportional* increase in the clearance rate—say from 20-22% or from 40-44% depending on the initial clearance rate, both of which represent 10% increases. In this case, the appropriate dependent variable would be the natural log of the relevant clearance rate.

65. This has the added advantage of providing more immediately obvious comparisons across regressions, since the estimated coefficient on the time dummy that Cassell and Fowles refer to as "*MIRANDA*", and which I refer to as "Post-1966," will then yield the post-1966 percentage reduction in the clearance rate for all the various crime categories. For example, using the actual clearance rates as the explanatory variable, as Cassell and Fowles did, means that a drop of, say, 2% in the clearance rate for property crime cannot be immediately compared with a drop of 2% in the clearance rate for violent crime because the initial clearance rates are much larger for violent crimes.

66. While Cassell and Fowles used total police personnel as their explanatory variable, I used a slightly different measure that tried to capture the number of officers. The reason for this is that the proportion of civilian personnel in urban police departments has been growing: from 6% in 1950 to 22% in 1995. My assumption is that the number of officers is a better measure of the crime-fighting resources of the police than the number of total police employees. Accordingly, I computed the ratio of police officers to total police employees for each year from 1950-1995 and then multiplied this ratio by Cassell and Fowles' measures of police personnel and monetary resources.

rates but the *ratio* of police resources to crime that is important as well.[67] For example, Schulhofer has argued that it would not be surprising to see a drop in the violent crime clearance rate in the second half of the 1960s given the fact that the number of police officers per 100 violent crimes fell from 80 in 1965, just prior to the decision in *Miranda*, to only 45 in 1970.[68] As we will see below, the ratio of police resources to crime is indeed important, and the regression results are, at times, quite sensitive to the measure of crime that is used in the denominator of this ratio.

In addition to these, however, Cassell and Fowles include a number of variables that one might expect to affect crime rates, but which have no obvious relationship to clearance rates. To this category I would add the measure of the fifteen- to twenty-four-year old population (the most crime-prone age group), labor force participation and unemployment rates, per capita income, and number of births to unmarried women.[69] Since the model already controls for crime, I decided to jettison these variables to simplify the model. The precise definition and data sources of all the variables that I use in my regressions are set forth in the Appendix.

I would have liked to control for the nature and type of crimes for each year of the study period. For example, spousal murder is far more easily cleared than murders committed during drug turf wars. Clearly, some measure of the magnitude of the illegal drug market would have been helpful, but none is readily available. Has the growth of drug dealing by criminal gangs increased the number of arrests per crime? Presumably, if police have to process many defendants to clear one crime, their ability to clear crimes will fall. Moreover, attitudes toward police both in the general public and among criminals might have an impact on clearance rates, but again, these variables are not easily reduced to statistical quantities. Even issues such as the proportion of arrested individuals testing positive for drugs or carrying handguns could influence clearance rates, since these factors delay and impede police effectiveness: The more time the police have to spend dealing with difficult individuals, the less time they have for solving crimes.

With so many factors that influence national clearance rates missing from the regression model, the danger that the effect of other influences will be wrongly attributed to *Miranda* is a serious concern. As did Cassell and Fowles, I attempted to compensate by including some trend measures which should adequately control for influences on clearance rates that change smoothly over time.[70] Note, though, that if any of the above-mentioned omitted variables experienced an abrupt shift to a different regime—such as from a world of modest demand for illegal substances to one of pervasive illegal drug markets and their attendant criminal gang activity, or from a world where criminal defendants are frequently not represented by counsel to one in which they are always so represented—then the time trends will not capture

67. *See* Stephen J. Schulhofer, *Pointing in the Wrong Direction*, LEGAL TIMES, Aug. 12, 1996, at 21.

68. *See id.*

69. *See* Cassell & Fowles, *supra* note 2, at 1082.

70. I controlled for trends with linear and quadratic time trend terms (time and time-squared).

the shift and the influence of the regime-change will inappropriately be attributed to *Miranda*.

B. *Regression Results*

1. *My basic model of violent crime clearance rates.*

Measured violent crime clearance rates are roughly one-third lower during the 1966-1995 period than they were from 1950-1966.[71] Assuming that no forces were operating to *raise* clearance rates in the period after mid-1966, this 33% drop represents the upper-bound estimate on the impact of *Miranda* since this figure captures all of the effects that we have discussed above that would cause clearance rates to fall. Of course, we also know that there was a downward trend in violent crime clearance rates prior to *Miranda*, so we must be careful not to attribute this trend to *Miranda*. When we remove the effect of this downward time trend, the magnitude of the post-mid-1966 drop in clearance rates is reduced to about 25%, which then becomes our new upper-bound estimate of the effect of *Miranda*.[72] When additional explanatory variables are introduced into our estimates of the violent crime clearance rates, we would expect the estimate of the unexplained post-mid-1966 drop in clearance rates to shrink further still. Whatever is left, assuming that the data is accurate and the regression specification is appropriate, would then be the estimated unexplained average amount by which violent crime clearance rates were lower post-mid-1966 than they were from 1950 to mid-1966.

Tables I and II present regressions for the violent crime clearance rates using the explanatory variables discussed above.[73] The one difference be-

---

71. I obtained this result by regressing the *ln(violent crime clearance rate)* on a constant and a post-mid-1966 time dummy. The coefficient on the time dummy was estimated to be -.333, which suggests that from mid-1966 to 1995 measured violent crime clearances were 33% lower, on average, than they were from 1950 to mid-1966.

72. The regression that removed the effect of the downward time trend from the estimate of the post-1966 change in clearance rates was: *ln(violent crime clearance rate) = Constant - .25*(Post-1966 Time Dummy) - .0045*(time in years since 1949) + .000023*(time in years since 1949)*^{2}. The negative coefficients on the "time in years" variable implies that there was an overall downward trend in violent crime clearance rates during the entire 1950-1995 period. The positive coefficient on the time-squared term implies that the rate of decline in the violent crime clearance rate was diminishing or, equivalently, that the downward trend was leveling off.

73. In every regression in Tables I and II, the Durbin-Watson statistic suggested that serial correlation was present, a common problem in time-series analyses. Accordingly, all of the regressions presented in this paper were estimated using the Hildreth-Lu correction for serial correlation. Serial correlation occurs when the following condition holds: The fact that regression estimates are over (or under) the true value in a given year implies a greater likelihood that the regression estimates for succeeding years will also be over (or under) the true value. Thus, serial correlation is present if an overprediction in the regression estimate of the clearance rate in, say, 1990 suggests that the regression estimate will tend to overpredict the clearance rate in the following year. The problem typically occurs because the cumulative effects of omitted variables tend to persist over time. *See* ROBERT S. PINDYCK & DANIEL L. RUBINFELD, ECONOMETRIC MODELS AND ECONOMIC FORECASTS 137-38 (3d ed. 1991).

There are a number of different techniques for dealing with serial correlation in addition to the Hildreth-Lu method that I used throughout this paper. When I ran the regressions in Tables IV and V using one of the other major techniques—the Prais-Winsten correction for serial correlation—the post-1966 coefficient was always *positive* but statistically insignificant. In the Prais-Winsten regres-

tween these two tables is that Table I proxies the crime rate with the violent crime rate, whereas Table II proxies the crime rate with the murder rate. Within each table, a number of different regression specifications are used, and two different post-1966 time dummies are employed.[74] The upper panel of Table I assumes that the effect of *Miranda* is immediate and constant in percentage terms from the middle of 1966 through the end of the data period. The bottom panel assumes that only two-thirds of the effect of *Miranda* was felt for the first eighteen months, and then its full and constant effect was felt for the remainder of the data period.

Looking at the first column of the top panel of Table I, we see that the time dummy identified as "Post-1966" is associated with a 5.6% decrease in the violent crime clearance rate. The table notes, however, that this coefficient is not statistically significant at any conventional level.[75] This regression suggests that most of the post-1966 downward deviation from trend in violent crime clearance rates is explained by the fall in police resources per violent crime, analogous to the Cassell-Fowles variable "dollar capacity (real)."[76]

The second through fourth columns of Table I include various combinations of the four measures of police personnel and monetary resources as explanatory variables. When all four of these measures are included in column 2, the coefficient on the post-1966 dummy rises to -7.8% and becomes statistically significant at the .05 level. None of the four regressions in the top panel of Table I is entirely satisfactory. Columns 1 and 2 are problematic in that more police officers are associated with a significantly *lower* clearance rate, which is almost certainly incorrect. Columns 3 and 4 avoid the erroneous signs on the various police measures by eliminating all but one in each case, but have lower R-squared values. In the end, not having a strong reason to choose among them, I opted for the objective rule of preferring the regression with the highest R-squared value, which was the column 2 regression.

---

sions, the time trends and the crime in small cities variable explained all of the pattern of the clearance rate changes, leaving no post-1966 effect. If the Prais-Winsten results are correct, then the Cassell-Fowles hypothesis would be directly disconfirmed by the statistical data. As further confirmation that econometrics is as much art as science, I chose to reject the Prais-Winsten results because (1) they seem too far out of line with the OLS and Hildreth-Lu results and (2) for the violent crimes, though not the property crimes, the Prais-Winsten correction seemed to elevate the Durbin-Watson statistic too far above 2, suggesting that the correction was not working properly. The Prais-Winsten results underscore, however, the difficulty of accurate estimation of time series data when so many of the explanatory variables are trending together.

74. Cassell and Fowles refer to their various post-mid-1966 time dummies as "*MIRANDA*," whereas Table I uses the more neutral short-hand term "Post-1966." *See* Cassell & Fowles, *supra* note 2, at 1081. My label simply reflects the fact that this dummy will capture the effect of any post-mid-1966 influence that does not change smoothly enough to be captured in the time-trend variable or is not otherwise controlled for by the other explanatory variables. The precise definitions of the particular versions of "Post-1966" are specified in each table.

75. The lack of statistical significance implies that the estimated "post-1966" effect could have simply been caused by chance. If the probability of an observed effect being caused by chance is "sufficiently low," then the observed effect is deemed statistically significant. The standard for what constitutes a sufficiently low probability is conventionally .05, but can also be .01 or .10. *See* DAMODAR GINJARTI, ESSENTIALS OF ECONOMETRICS 103-04 (1992).

76. *See* Cassell & Fowles, *supra* note 2, at 1078.

Consequently, in the discussion that follows, I only report regression results using the column 2 explanatory variables.

The lower panel of Table I presents the regression results under the assumption that *Miranda*, or whatever causal factor is proxied by the time dummy, had only a two-thirds effect for eighteen months and the full effect thereafter. Note that the small improvement in the R-squared value for the lower panel column 2 regression suggests a slightly better fit with this model than with the model showing an immediate and full effect starting in mid-1966 (the top panel of Table I).[77] In both the top and bottom panels of Table I, we observe that the post-1966 coefficient is always negative and that in six of the eight regressions it is statistically significant. This shows that, while my specification is somewhat different than that employed by Cassell and Fowles, my changes did not affect the robustness of their regression results for violent crime clearance rates. Taking column 2 as the preferred basic model, the downward deviation in the violent crime clearance rate from trend that Cassell and Fowles attribute to *Miranda*, but which also includes the effect of any of the omitted variables discussed above, falls roughly 8-12%. Since the simple regression that includes only the post-1966 time dummy and the two time trends (time and time-squared) generates a predicted post-1966 effect of -25%, adding the independent variables in Table I explains roughly one-half to two-thirds of the total post-1966 deviation from trend in the clearance rate.

Table II presents the same regressions as Table I, with the single exception of the change in the proxy measure of crime. Thus, whereas Table I uses the violent crime rate, Table II uses the murder rate. Moreover, whereas Table I computes the number of officers per violent crime and the police expenditures per violent crime, Table II instead computes the number of officers per murder and the number of police expenditures per murder. As one can see, the magnitude and significance of the post-1966 dummy coefficients are greater in Table II than in Table I—at times substantially. Obviously, the Cassell-Fowles hypothesis is strengthened by larger post-1966 dummy coefficients, so we must evaluate whether the murder rate or the violent crime rate is the better explanatory variable.

Recall that the crime proxy is being used to capture the influence of increased crime on the ability of police to clear cases. Since murder is the best-measured crime, and since other crime measures have swollen over time owing to better recording rather than to actual crime increases, my initial thought was that the murder rate was the better variable to reflect the actual burdens on the police. On the other hand, some of the decrease in the clearance rates is likely to be the result of the swelling in the police recording of crime, which brings in less important, less reliable, and less solvable crimes. Therefore, if we are explaining a variable tainted by artificial swelling—the denominator of the clearance rate is growing artificially over time—it may be more appropriate to include the explanatory variable that is likely growing at the same rate.

77. For a description of the R-squared value, see text accompanying note 65 *supra*; *see also* THAD W. MIRER, ECONOMIC STATISTICS AND ECONOMETRICS 91-94 (3d ed. 1993) (describing computation of R-squared).

In fact, if one uses the adjusted R-squared measure as the determinant of the best model, then the violent crime measure wins decisively, since the adjusted R-squared is always higher in Table I than in Table II. Using the same standard, the second column and bottom panel of Table I provide the best regression, and I will rely on this model for the remainder of the paper.

2. *Multicollinearity problems.*

Multiple regression is a technique used to identify the effect on the dependent variable (in our case the clearance rate) of a unit change in an explanatory variable (most significantly here the post-1966 dummy), while holding every other explanatory variable constant. If there is a high degree of collinearity between the explanatory variables, it becomes impossible to hold other explanatory variables constant: Any time one explanatory variable changes, the other explanatory variables with which it is highly correlated will also change. This problem, known as multicollinearity, may prevent the regression from providing a proper estimate of the effect of any of the collinear explanatory variables.

As it turns out, there is a high degree of correlation between some of the most important explanatory variables in these clearance rate regression models. Table III shows the correlation between various explanatory variables from Tables I and II, which in certain cases is extremely high. For example, the correlation between police expenditures per capita and the violent crime rate is .97, and the correlation between officers per violent crime and police expenditures per violent crime is .92. This problem of multicollinearity undoubtedly explains why the estimated coefficients on these variables in Tables I and II of the Cassell-Fowles paper frequently have the wrong sign. For example, Cassell and Fowles' Table I suggests that increasing the number of police officers per capita and the ratio of police resources to crime, which we would expect to improve the clearance rate, in fact tends to depress it.[78] A similar multicollinearity problem appears in the column 2 regression of my Table I: Even though two of the four measures of police resources are associated with an improved clearance rate and the third shows virtually no effect, the ratio of police employees to the number of violent crimes, analogous to the Cassell-Fowles variable "officer capacity," appears to have a *negative* effect on the clearance rate. Moreover, this coefficient estimate has the highest t-statistic in both the column 2 regressions.[79]

---

78. *See* Cassell & Fowles, *supra* note 2, at 1083 tbl.I. Another severe multicollinearity problem exists between the "urbanization" and "crime in small cities" variables that are used by Cassell and Fowles. The R-squared value between these two variables is .98. Since the FBI data on clearance rates was only for cities, it was unclear to me why the urbanization variable should be included at all. Therefore, I omitted it from my regressions.

79. The regression model provides an estimate of the effect of each explanatory variable (the estimated coefficient) and a measure of the precision of that estimate (the standard error). The ratio of the estimated coefficient to the standard error is called the t-statistic, which is used to ascertain the statistical significance of the estimated coefficient. In brief, the t-statistic can be used to determine the likelihood that a coefficient estimate of a particular size was generated by chance when the true value was zero. The higher the value of the t-statistic, the smaller the likelihood that the observed coefficient is simply a random variation from a true zero value. A rough rule of thumb is that a t-

Given the fact that we are most interested in the coefficient estimate for the post-1966 dummy, it is troubling to find a high correlation between the post-1966 variable and police expenditures per capita as well as between the post-1966 variable and the violent crime rate.[80] While the coefficient estimates for these collinear variables will remain unbiased—in the statistical sense that if we could analyze new data and secure many coefficient estimates, the mean of these estimates would yield the true value—our confidence in these estimates is impaired.

3. *The onset and duration of the "*Miranda *effect."*

The statistical significance of the post-1966 coefficient does not reveal whether a similar time dummy starting either a few years before or after 1966 would provide a better fit. To explore this possibility, I ran the column 2 regressions for both the top and bottom panels of Table I, while varying the starting date from 1962 through 1969. For the bottom panel, a 1966 starting date did in fact provide both the most significant post-1966 coefficient and the highest R-squared. For the top panel, however, the best fit came from starting the dummy in mid-1967. These findings imply that a model in which the effect in question starts immediately in mid-1966 is slightly less successful than one in which the effect on clearance rates starts in mid-1967 (adjusted top panel) or, one in which the effect is nonexistent for the last six months of 1966, only half the full effect in 1967, with the full effect coming in 1968. Finally, the regression based on *Miranda* having a two-thirds effect for the first 18 months beginning in mid-1966 (original bottom panel) is slightly better still.[81]

Note that thus far all of the regression models have implicitly assumed that once the *Miranda* effect has taken hold, it continues indefinitely at a constant rate. However, as Cassell and Fowles observe, we have no assurance that this approach would necessarily capture the influence of a Supreme Court decision.[82] Rather, it is conceivable that *Miranda* caused some disruption at first, but that once the police learned how to respond to *Miranda*'s restrictions, the decision ceased to have an effect on measured clearance rates. To explore this possibility, I ran the same regressions as in Table I, but tried yet another variation in the post-1966 dummy: In this variant, I assumed that

---

statistic in excess of two is considered to be statistically significant at customary levels of confidence.

80. The correlation between the two post-1966 variables and police expenditures per capita is .88 for post-1966(1) and .88 for post-1966(2). The correlation between the violent crime rate and post-1966(1) is .90.

81. These conclusions about the relative performance of the three models—that the model in which *Miranda* has only a two-thirds effect for 18 months is somewhat superior to the model in which post-1966 has no effect for one year, which in turn is superior to the model in which *Miranda* takes full effect in mid-1966—also hold if one drops the time-squared term from the regression. In general, dropping the time-squared term increases the size of the post-1966 effect on clearance rates. For example, while the regression in the first column and bottom panel of Table I shows a post-1966 effect of -7% with a t-statistic of -1.675, simply dropping the time-squared term from this regression causes the post-1966 coefficient to jump to -9.4% with a t-statistic of -1.76.

82. *See* Cassell & Fowles, *supra* note 2, at 1092-95.

Miranda had a disruptive effect on police that was immediate and complete in mid-1966 but fell to two-thirds by 1969, to one-third by 1970, and to zero thereafter. The post-1966 effect for all four regressions in Table I fell sharply to around minus 2-4%. In every case, both the adjusted R-squared and the t-statistic on the post-1966 dummy were higher for the Table I regressions than for the corresponding regression using this "short-lived *Miranda* effect" dummy. These results indicate that the deviation from time trend in clearance rates that occurred in the 1960s probably took full effect some time after *Miranda* was decided and has remained persistent.[83]

4. *The changing composition of reporting cities.*

In the earlier discussion of the FBI data, I noted that the clearance rate data for the period from 1950-1995 is not for a consistent set of cities. This variation is not simply reflective of the changing urban composition of the population, but also reflects substantial differences in which cities report. Thus, in 1950, only 1601 cities with a population of 54.7 million are included in the clearance rate data, whereas in the peak year of 1981, 9179 cities with a population of 146.2 million reported, and in 1995, 8278 cities with a population of 148.3 million reported.[84] This introduces a degree of uncertainty into the analysis because we can never be sure from looking at the aggregate national data whether the clearance rates are changing because of some real phenomenon or just because a different set of cities is reporting. A second complication is that over time there has been a vast increase in the total population of the cities that report, but the FBI simply compiles all of this information as "annual clearance rates" with no adjustment for the changes in cities reporting. To give some sense of the scope of the problem, I computed the ratio of total population of cities reporting to the FBI to the entire United States population. This ratio starts off at .36 in 1950, rises to .46 in 1960 before dropping back to .41 in 1961, rises to .51 in 1966, peaks at .64 in 1981, before falling to .56 in 1995.[85]

In addition to the problem that the clearance rate measures are from a different and generally expanding set of cities over time, the FBI statistics for numbers of police officer and police resources come from yet different and still changing cities. For example, these police measures came from 3223 cities with a population of 73.3 million in 1950, but from 10,010 cities with a

83. This conclusion is supported by additional tests performed by Cassell and Fowles. *See* Cassell & Fowles, *supra* note 2, at 1099 tbl.V.

84. *See* UCR-1950, *supra* note 5, at 42 tbl.13; UCR-1981, *supra* note 5, at 153 tbl.19; UCR-1995, *supra* note 5, at 199 tbl.25.

85. *See* UCR-1951, *supra* note 5, at 42 tbl.13 (reporting cities' population 54,690,170); UCR-1961, *supra* note 5, at 79 tbl.5 (reporting cities' population 95,538,127); UCR-1966, *supra* note 5, at 102 tbl.13 (reporting cities' population 99,371,000); UCR-1981, *supra* note 5, at 153 tbl.19 (reporting cities' population 146,165,000); UCR-1995, *supra* note 5, at 199 tbl.25 (reporting cities' population 148,295,000). The total resident U.S. population was 151,868,000 in 1950, 182,992,000 in 1961, 195,576,000 in 1966, 229,466,000 in 1981, and 262,755,000 in 1995. *See* BUREAU OF CENSUS, U.S. DEP'T OF COMMERCE, STATISTICAL ABSTRACT OF THE UNITED STATES 8 tbl.2 (1996).

population of 164.3 million in 1995.[86] Thus, we are explaining clearance rates from one set of changing cities with data on police resources from a different set of changing cities. Moreover, the explanatory variable indicating the proportion of violent crimes committed in cities with fewer than 250,000 inhabitants comes from yet another changing set of cities.[87] Furthermore, when we create the variables for police officers per murder or per violent crime, the crime figures come from a different set of cities than the police officer data, which again muddies the explanatory variable. Finally, for certain key variables—the clearance rates for all violent crimes and for all property crimes, and the violent crime rate—the pre-1975 data comes from *UCR* data adjusted by James Alan Fox,[88] while the post-1975 data is unadjusted.

The best way to control for this effect would be to find the core set of cities that consistently report over a period of time and limit the analysis to those cities. This task requires getting FBI data tapes and then performing a great deal of data manipulation for forty-five years of data, an exercise which was not practicable given the limited time available for this comment. The effect of these data shortcomings on the regression results is unknown, but it could be substantial, which again counsels caution in interpreting these results.

5. *Simultaneity problems.*

Ordinary least squares regression assumes no simultaneity, meaning that the independent variables influence the dependent variable and that the dependent variable does not in turn influence any of the independent variables. Our model of clearance rates almost certainly presents a simultaneity problem. If in fact *real*, as opposed to merely reported, clearance rates fell over time, then one would expect that crime would be affected by the reduction in police effectiveness. But crime is one of the independent variables we are using to explain clearance rates, which means that we have a classic simultaneity problem. As Cassell and Fowles note, this can bias the regression estimates in unpredictable ways.[89] They have informed me that they will try to address this issue in a future paper, and they asked me to await those results before exploring this issue further using their data.[90]

6. *Results for other crimes.*

With the constant caveats about data quality and multicollinearity, the difficulties of national time-series estimation, and the problem of simultaneity, the preceding Tables I and II regressions suggest that *something* other than the

86. *See* UCR-1950, *supra* note 5, at 20 tbl.11; UCR-1995, *supra* note 5, at 279 tbl.70.

87. *See* Appendix.

88. *See* JAMES ALAN FOX, FORECASTING CRIME DATA: AN ECONOMIC ANALYSIS 81-86 & 83 tbl.A-1 (1978).

89. *See* Cassell & Fowles, *supra* note 2, at 1101-03.

90. Another simultaneity problem occurs because one would expect a negative relationship between crime and clearance rates if there is measurement error in the crime statistics, as there clearly is. It is unclear whether this bias would influence the estimated coefficient on the post-1966 dummy.

factors controlled for in the various regressions happened in the mid-1960s to depress violent crime clearance rates. Tables IV and V use the regression model in column 2 of the bottom panel of Table I to examine clearance rates for other crimes. In general, these results are weaker than those shown by Cassell and Fowles. While all the coefficients on the post-1966 dummy in Tables IV and V are negative, reflecting a drop in clearance rates, in only two of the nine regressions (total violent crime and larceny) is the coefficient significant at the conventional .05 level of significance. Moreover, the larceny regression does not seem to fit the data well. While every other regression in Tables I, II, IV, and V has an R-squared in excess of .9, the larceny regression has an R-squared of only .61, which suggests that that particular equation may not be well-measured. In contrast, Cassell and Fowles report a post-1966 effect that is negative and statistically significant at the .05 level in six of the nine regressions, for total violent crime, robbery, and all four property crimes.[91] The evidence from Tables IV and V taken as a whole would seem to suggest a statistically significant post-1966 drop in clearance rates for total violent crimes, but not for any of the four individual violent crimes that comprise the total, and a second statistically significant effect for larceny, but not for any other property crime or total property crime, albeit in a flawed regression that might undermine our confidence in that estimated post-1966 effect. On the other hand, all of the Table IV and Table V coefficients are negative, four of the nine are significant at the .10 level or better, and for robbery (-12.3%), larceny (-13.3%), and vehicle theft (-15.0%) the post-1966 effect is substantial. This pattern may suggest that the post-1966 effect is greatest for those crimes for which serious offenders may be involved in scores or even hundreds of crimes. Police interrogation for these offenses could conceivably reveal habitual offenders, "clearing" many cases beyond the one which led to the initial arrest.

CONCLUSION

The motto of the Royal Society of London is *nullius in verba*: Trust not in words.[92] Data warrants greater respect than opinion and verbiage.[93] Cas-

91. *See* Cassell & Fowles, *supra* note 2, at 1083 tbl.I & 1086 tbl.II. When I ran the regression model from column 2, bottom panel of Table II—which used murder rather than violent crime as the crime proxy explanatory variable—five of the nine regressions had post-1966 effects that were negative and significant at the .05 level (total violent crime, robbery, aggravated assault, total property crime, and vehicle theft). In every case, however, the adjusted R-squared values from these regressions were lower than the comparable values from the Table IV and Table V regressions (using the violent crime rate as the crime proxy). Accordingly, the murder-proxy regression results are not presented but are available from the author.

92. Steve Jones, *The Set Within the Skull*, N.Y. REV. BOOKS, Nov. 6, 1997, at 13.

93. Yale Kamisar has discussed how certain police personnel in the past have been willing to blame Supreme Court decisions for declines in crime clearance rates without an adequate factual basis. For example, in 1962, the chief of police in the District of Columbia blamed a 1957 decision barring confessions elicited during unreasonably prolonged precomittment detention for a 9 percentage point decline in clearance rates. In fact, Kamisar noted, the clearance rate dropped 7 percentage points in the two years *before* the decision, *rose* 3 percentage points in the two years immediately *following* the decision, and then fell 12 percentage points over the next three years. *See* Kamisar, *Tactics*, *supra* note 18, at 466-67.

sell and Fowles are to be commended for trying to go beyond mere words to test the impact of an important Supreme Court decision on the effectiveness of our criminal justice system. Their statistical study is a model of clarity in exposition, and the authors deserve credit for their willingness to share their data. Based on my review of all the statistical evidence, I would venture that there is some evidence that the *measured* violent crime clearance rate is 10-12% lower in the post-mid-1966 period than would have been expected based on the various time-series regression models. Since this finding is fairly robust at a high degree of statistical significance across a number of models in Tables I and II, I consider this to be noteworthy but not dispositive evidence of such an effect on the *measured* violent crime clearance rate. The evidence of such a downward post-1966 effect for specific violent crimes or for any property crime is necessarily weaker. The post-1966 effect on the clearance rate for murder, rape, robbery, assault, all property crimes, burglary, and vehicle theft, while negative in each case, is never significant at the .05 level. A statistically significant post-1966 effect on clearance rates is found only for total violent crimes and larceny, and the substantially lower R-squared value on this latter equation may cast doubt on that result. These results do not provide a ringing endorsement of the Cassell-Fowles hypothesis. That seven of the nine regressions in Tables IV and V show no significant effect on the post-1966 dummy variable suggests that the title of the Cassell-Fowles rejoinder—"Falling Clearance Rates After *Miranda*: Coincidence or Consequence?"—may be omitting a third plausible possibility. For seven of nine crime categories, the falling clearance rates are fully explained by higher crime, relatively fewer police resources, and a long-term downward trend that started in 1950. Only for the violent crime clearance rate do we consistently—although even here not invariably[94]—observe a statistically significant post-1966 drop in clearance rates that is not explained by the above long-term forces.[95]

I have already pointed out that stronger results in support of the Cassell-Fowles hypothesis could be obtained using a different proxy for the crime rate—the murder rate rather than the violent crime rate—but that using the

94. The two regressions in the first column of Table I showed no statistically significant post-1966 effect on the violent crime clearance rates, even though all the other regressions in Tables I and II did show such an effect on the violent crime clearance rate.

95. Cassell and Fowles note in their rejoinder that the regression results are more supportive of the Cassell-Fowles thesis if one drops the time-squared term from the regression, since this change increases the number of post-1966 effects that are statistically significant at the .05 level or better from two out of nine (violent crime and larceny) to three out of nine (violent crime, property crime, and vehicle theft). In addition, this change brings the robbery clearance rate to marginal significance. But of the four variables whose significance changes by dropping the time-squared term, two have a higher R-squared and two have a lower R-squared, so the regression results themselves do not immediately reveal which is the better specification. Moreover, I included the time-squared term since it seemed unlikely to me that a proportion variable, such as a clearance rate, would be likely to trend down at a linear rate for the roughly half century covered by our data. Since the clearance rate is a proportion, it is bounded by zero below, and therefore any strong downward trend would ultimately approach zero asymptotically—which a linear trend could not do. This suggests that perhaps a log-odds ratio, which constrained the predicted clearance rate to fall between 0 and 1, might be preferable to using the actual clearance rate as the dependent variable. Making this change led to the same results discussed above in this footnote.

latter measure invariably led to higher adjusted R-squared values.[96] In their rejoinder, Cassell and Fowles suggest that a third, even broader, measure – all property and violent crimes– is preferable to the narrower violent crime rate in proxying the workload of the police. This broader measure of so-called "index crimes" will largely be a property crime measure, since property crimes are so much more common than violent crimes, which means that the property crime figures drive this proxy.[97] The perfect measure would weight how much time the police spend on each type of crime, so that a murder would be given a much greater weight than a theft of a car radio. Certainly, if the police spent the same amount of time on each crime and if the property crime component was at least as reliably measured as the violent crime component, then the index crime figure would be preferable as a proxy for the workload of the police. But neither of these conditions holds.[98] Still, it is useful to see how sensitive the results are to different specifications, and the thrust of my analysis is that the Cassell-Fowles results are not robust at the .05 level of significance to reasonable changes in their specification.[99] Nonetheless, the consistency of the size and signs of the post-1966 effects, particularly for the violent crime variable, does provide some evidence in support of an unexplained post-*Miranda* downward deviation from trend in various clearance rates.

But some evidence of a drop in measured clearance rates is a long way from proof of a statistically significant drop in actual clearance rates caused by the Supreme Court's *Miranda* decision. First, one must necessarily be wary of the conclusions of an interrupted time-series analysis on national clearance rate data. This is particularly true when, as here, the high degree of correlation between the explanatory variables undermines the reliability of the estimated post-1966 effect. Second, before I could endorse even the limited conclusion that there has been a post-1966 downward deviation from trend in violent crime clearance rates, I would want to see the results of an analysis of a consistent set of cities over time, which might permit use of the more reliable method of panel data analysis. Indeed, with clearance rate data coming from different but growing numbers of cities over time, with data for the ex-

---

96. *See* note 91, *supra*.

97. In 1995, total FBI index crimes came to 13.9 million, of which 12.1 million were property crimes and 1.8 million were violent crimes. UCR-1995, *supra* note 5 at 5, 10, 35. Thus, property crimes were 87% of total index crimes.

98. Moreover, as can be seen in Table I of the Cassell-Fowles rejoinder, the index measure does not invariably lead to a higher R-squared than the violent crime measure (compare the first and fifth rows of that table). When the index crime measure is used, the post-1966 effect on three crime categories that had been insignificant in my regressions becomes statistically significant at the .05 level—robbery, property crimes, and vehicle thefts. But in two of these three cases—property and vehicle theft—the adjusted R-square value fell when the index crime measure was used instead of the violent crime measure that I present in my Tables IV and V.

99. Cassell and Fowles also demonstrate in Table I of their rejoinder that the number of statistically significant post-1966 clearance rate effects jumps from two of nine to four of nine (robbery and property become significant) when all police employees rather than simply all officers is used as an explanatory variable. If I had to choose one measure, I would probably prefer officers as a better measure of the crime-solving capacity of the police department, but it is interesting to see the results for both measures. Note, too, that for the two crime categories that become significant with the switch, both R-squared values are higher using the Cassell-Fowles measure.

planatory variables coming from yet a different set of cities, and given the generally questionable nature of FBI crime data (particularly in the 1950s and 1960s) and the potentially serious problems of multicollinearity and simultaneity, one is left with an unbridgeable uncertainty about how much confidence to repose in any of the statistical results.

Even if we were to find a downward deviation from prior trends in clearance rates, causal attribution to *Miranda* is difficult in light of the vast systemic changes both in crime and criminal justice that were occurring and have persisted since the mid-1960s. My sense is that there has been some drop in *actual* clearance rates owing to the dramatic changes in the nature of crime, drugs, and attitudes toward authority that emerged in the late 1960s, as well as to the changes in the criminal justice system ushered in by the Warren Court's many decisions in this area, not just *Miranda*. Moreover, *measured* clearance rates have probably dropped also as a result of the improved quality and reliability of crime and clearance rate data. We must query how much of the measured deviation from trend found in the regressions would remain once we subtracted out the effect of these factors.[100]

Finally, while statistical methods are essential to the task undertaken by Cassell and Fowles, they can be only one component of the analysis. Other types of direct evidence of the effect of *Miranda* on clearance rates, such as that developed elsewhere by Cassell, Schulhofer, and numerous other scholars, are essential to interpret regression results that satisfy appropriate statistical standards. If this other evidence provided strong support for the Cassell-Fowles hypothesis, then generally corroborative empirical evidence would carry greater weight than it would without such support. Since I have not studied this substantial literature with the requisite care, I leave it to others to make the ultimate judgment of whether this other evidence supports the claims of Cassell and Fowles—an issue about which there is substantial controversy.[101] It does seem, however, that if there were a *Miranda*-induced drop in clearance rates, it would most likely come from what I referred to as the "other-crimes effect" since in most cases the *Miranda* warnings come only after the case is cleared by arrest. If so, the costs of *Miranda* might be far less than its critics fear.

---

100. Cassell and Fowles doubt that the factors that I mention could cause an abrupt drop in the clearance rate of the magnitude reflected in the post-1966 dummy coefficient. But this coefficient simply captures the average unexplained effect during the entire post-1966 period, which was not necessarily immediately achieved with full effect. In fact, I ran a regression of the violent crime clearance rate on a succession of year dummies and found that this rate is over 5% lower in 1964 and 1965 than it had been from 1950-1963, drops an additional 7% in 1966, drops an additional 8% in 1967, drops an additional 8% in 1968 and a further 2% in 1969 before leveling out. Thus, it is not the case that the clearance rate drop is a single precipitous decline. It seems plausible that such a phased-in decline in clearance rates could result from improved recordkeeping and greater professionalization of police forces.

101. Yale Kamisar provides a useful summary of some of the contending views. *See* YALE KAMISAR, WAYNE R. LAFAVE & JEROLD H. ISRAEL, MODERN CRIMINAL PROCEDURE: CASES, COMMENTS & QUESTIONS 59-81 (8th ed. Supp. 1997) (examining the practical impact of *Miranda*).

TABLE I. VIOLENT CRIME CLEARANCE RATES FOR CITIES (1950-1995)
Using Violent Crime Rate As an Explanatory Variable (*t* statistics in parenthesis)

Post-1966: 1966=.5 and 1967 and on=1				
Variables	**Violent Crime**			
POST-1966	-.056 (-1.658)	-.078 (-2.432)*	-.108 (-3.266)**	-.119 (-3.755)**
VIOLENT CRIME RATE	.003 (0.185)	-.028 (-1.449)	-.026 (-1.412)	-.041 (-2.673)*
OFFICERS (PER CAPITA)	-.194 (-2.810)**	-.326 (-4.153)**		
OFFICERS/ NUMBER OF VIOLENT CRIMES		.408 (2.333)*		.281 (3.161)**
POLICE EXPENDITURES (PER CAPITA)		.007 (2.839)*		
POLICE EXPENDITURES/ NUMBER OF VIOLENT CRIMES	.025 (4.984)**	.001 (0.089)	.016 (3.348)**	
CRIME IN SMALL CITIES[a]	.004 (2.445)*	.001 (0.809)	.003 (1.456)	.001 (0.704)
TIME	-.011 (-3.690)**	.001 (0.194)	-.009 (-2.143)*	.005 (1.429)
TIME^2	.0002 (4.395)**	.000004 (0.050)	.0002 (2.880)**	.00001 (0.172)
INTERCEPT	3.945 (37.610)**	3.997 (33.329)**	3.846 (34.218)**	3.800 (28.818)**
Adjusted R^2	0.969	0.982	0.928	0.953
Post-1966: 1966=.33, 1967=.66 and 1968 and on=1				
Variables	**Violent Crime**			
POST-1966	-.070 (-1.675)	-.117 (-2.927)**	-.139 (-3.601)**	-.152 (-3.937)**
VIOLENT CRIME RATE	.004 (0.226)	-.034 (-1.795)†	-.022 (-1.239)	-.039 (-2.405)*
OFFICERS (PER CAPITA)	-.177 (-2.350)*	-.302 (-3.901)**		
OFFICERS/NUMBER OF VIOLENT CRIMES		.482 (2.762)**		.224 (2.509)*
POLICE EXPENDITURES (PER CAPITA)		.008 (3.314)**		
POLICE EXPENDITURES/ NUMBER OF VIOLENT CRIMES	.024 (4.398)**	-.006 (-0.563)	.014 (2.938)**	
CRIME IN SMALL CITIES[a]	.003 (2.297)*	.0004 (0.257)	.002 (1.352)	.001 (0.753)
TIME	-.010 (-3.471)**	.004 (0.627)	-.007 (-1.800)†	.005 (1.089)
TIME^2	.0002 (3.883)**	-.00005 (-0.509)	.0001 (2.355)*	.000005 (0.068)
INTERCEPT	3.937 (37.531)**	3.990 (34.709)**	3.872 (34.755)**	3.856 (29.089)**
Adjusted R^2	0.968	0.983	0.931	0.927

** significant at .01 level * significant at .05 level † significant at .10 level

Note: The dependent variable is the natural log of the reported clearance rate. These regressions are estimated using the Hildreth-Lu correction for serial correlation. All police and resource measures have been multiplied by the ratio of officers to total employees.

[a] This variable measures the percent of violent crimes committed in cities with fewer than 250,000 inhabitants.

TABLE II. VIOLENT CRIME CLEARANCE RATES FOR CITIES (1950-1995)
Using Murder Rate As an Explanatory Variable (*t* statistics in parenthesis)

Post-1966: 1966=.5 and 1967 and on=1				
Variables	**Violent Crime**			
POST-1966	-.131 (-3.490)**	-.102 (-2.582)*	-.127 (-3.476)**	-.121 (-3.289)**
MURDER RATE	-.025 (-3.480)**	-.001 (-0.097)	-.024 (-3.659)**	-.018 (-2.099)*
OFFICERS (PER CAPITA)	.028 (0.349)	-.275 (-2.097)*		
OFFICERS/NUMBER OF MURDERS		1.379 (1.876)†		.355 (1.370)
POLICE EXPENDITURES (PER CAPITA)		.007 (3.136)**		
POLICE EXPENDITURES/ NUMBER OF MURDERS	.011 (0.949)	-.038 (-1.434)	.012 (1.118)	
CRIME IN SMALL CITIES [a]	.003 (1.289)	.001 (0.656)	.003 (1.386)	.002 (1.226)
TIME	-.005 (-1.272)	-.010 (-2.580)*	-.005 (-1.250)	-.005 (-1.131)
TIME^2	.00003 (0.519)	.00004 (0.873)	.00004 (0.535)	.00002 (0.383)
INTERCEPT	4.137 (33.596)**	4.020 (21.379)**	4.170 (57.552)**	4.047 (30.150)**
Adjusted R^2	0.899	0.951	0.897	0.904
Post-1966: 1966=.33, 1967=.66 and 1968 and on=1				
Variables	**Violent Crime**			
POST-1966	-.178 (-4.473)**	-.153 (-3.583)**	-.172 (-4.376)**	-.165 (-4.179)**
MURDER RATE	-.021 (-3.130)**	-.005 (-0.379)	-.020 (-3.173)**	-.015 (-1.828)†
OFFICERS (PER CAPITA)	.043 (0.571)	-.212 (-1.774)†		
OFFICERS/NUMBER OF MURDERS		1.025 (1.527)		.324 (1.335)
POLICE EXPENDITURES (PER CAPITA)		.007 (3.536)**		
POLICE EXPENDITURES/ NUMBER OF MURDERS	.010 (0.912)	-.028 (-1.152)	.012 (1.145)	
CRIME IN SMALL CITIES [a]	.002 (1.145)	.0009 (0.598)	.002 (1.277)	.002 (1.120)
TIME	-.004 (-0.904)	-.010 (-2.993)**	-.003 (-0.802)	-.003 (-0.687)
TIME^2	.00001 (0.197)	.00004 (0.939)	.00001 (0.187)	.000003 (0.045)
INTERCEPT	4.100 (35.359)**	4.048 (23.691)**	4.150 (60.808)**	4.040 (32.243)**
Adjusted R^2	0.917	0.965	0.910	0.916

** significant at .01 level * significant at .05 level † significant at .10 level

Note: The dependent variable is the natural log of the reported clearance rate. These regressions are estimated using the Hildreth-Lu correction for serial correlation. All police and resource measures have been multiplied by the ratio of officers to total employees.

[a] This variable measures the percent of violent crimes committed in cities with fewer than 250,000 inhabitants.

TABLE III. CORRELATIONS BETWEEN EXPLANATORY VARIABLES IN CLEARANCE RATE REGRESSIONS
Adjusted R^2 Values

Variables	POST-1996-1	VIOLENT CRIME RATE	OFFICERS (PER CAPITA)	OFFICERS/ VIOLENT CRIME	POLICE EXPENDITURES (PER CAPITA)	POLICE EXPENDITURES/ VIOLENT CRIME	MURDER RATE	OFFICERS/ MURDER	POLICE EXPENDITURES/ MURDER	CRIME IN SMALL CITIES	POST-1966-2
POST-1966-1		0.898	0.743	0.187	0.876	0.433	0.875	0.383	0.326	0.720	0.995
VIOLENT CRIME RATE	0.898		0.853	0.311	0.968	0.549	0.945	0.536	0.476	0.861	0.733
OFFICERS (PER CAPITA)	0.743	0.853		0.681	0.943	0.881	0.954	0.862	0.756	0.990	0.737
OFFICER/VIOLENT CRIME	0.187	0.311	0.681		0.448	0.918	0.483	0.933	0.801	0.673	0.179
POLICE EXPENDITURES (PER CAPITA)	0.876	0.968	0.943	0.448		0.701	0.982	0.677	0.608	0.936	0.878
POLICE EXPENDITURES/ VIOLENT CRIME	0.433	0.549	0.881	0.918	0.701		0.719	0.986	0.881	0.859	0.410
MURDER RATE	0.875	0.945	0.954	0.483	0.982	0.719		0.687	0.587	0.949	0.876
OFFICERS/MURDER	0.383	0.536	0.862	0.933	0.677	0.986	0.687		0.916	0.849	0.375
POLICE EXPENDITURES/ MURDER	0.326	0.476	0.756	0.801	0.608	0.881	0.587	0.916		0.729	0.321
CRIME IN SMALL CITIES[a]	0.720	0.861	0.990	0.673	0.936	0.859	0.949	0.849	0.729		0.718
POST-1966-2	0.995	0.733	0.737	0.179	0.878	0.410	0.876	0.375	0.321	0.718	

* Note: Post-1966-1 represents Miranda at .5 in 1966 and 1 for every year after. Post-1966-2 represents Miranda at .33 in 1966, .66 in 1967, and 1 for every year after that. All police and resource measures have been multiplied by the ratio of officers to total police employees.

[a] This variable measures the percent of violent crimes committed in cities with fewer than 250,000 inhabitants.

TABLE IV. TOTAL AND INDIVIDUAL VIOLENT CRIME CLEARANCE RATES FOR CITIES (1950-95)
Post-1966: 1966=.33, 1967=.66, 1968 and on=1
(*t* statistic in parenthesis)

Variable	Violent	Murder	Rape	Robbery	Assault
POST-1966	-.117 (-2.927)**	-.028 (-0.918)	-.033 (-0.539)	-.123 (-1.568)	-.026 (-0.626)
VIOLENT CRIME RATE	-.034 (-1.795)†	-.051 (-3.495)**	-.016 (-0.546)	.009 (0.239)	-.025 (-1.252)
OFFICERS (PER CAPITA)	-.302 (-3.901)**	-.029 (-0.496)	-.216 (-1.828)†	-.313 (-2.061)*	-.093 (-1.172)
OFFICERS/ NUMBER OF VIOLENT CRIMES	.482 (2.762)**	.186 (1.387)	.469 (1.765)†	.282 (0.825)	.212 (1.182)
POLICE EXPENDITURES (PER CAPITA)	.008 (3.314)**	.002 (1.286)	.006 (1.624)	.008 (1.655)	.003 (1.098)
POLICE EXPENDITURES/ NUMBER OF VIOLENT CRIMES	-.006 (-0.563)	-.014 (-1.635)	-.007 (-0.394)	.015 (0.674)	-.001 (-0.106)
CRIME IN SMALL CITIES[a]	.0004 (0.257)	-.004 (-3.070)**	-.004 (-1.656)	-.001 (-0.351)	-.0005 (-0.332)
TIME	.004 (0.627)	.008 (1.504)	-.012 (-1.121)	-.011 (-0.796)	.003 (0.431)
TIME^2	-.00005 (-0.509)	-.0002 (-2.624)*	.0002 (1.480)	.00005 (0.302)	-.0001 (-1.050)
INTERCEPT	3.990 (34.709)**	4.713 (53.456)**	4.334 (24.717)**	3.500 (15.533)**	4.235 (36.007)**
Adjusted R^2	0.983	0.985	0.968	0.964	0.968

** significant at .01 level * significant at .05 level † significant at .10 level

Note: The dependent variable is the natural log of the reported clearance rate. These regressions are estimated using the Hildreth-Lu correction for serial correlation. All police and resource measures have been multiplied by the ratio of officers to total employees.

[a] This variable measures the percent of violent crimes committed in cities with fewer than 250,000 inhabitants.

TABLE V. TOTAL AND INDIVIDUAL PROPERTY CRIME CLEARANCE RATES FOR CITIES (1950-95)
Post 1966: 1966=.33, 1967=.66, 1968 and on=1
(*t* statistic in parenthesis)

Variable	Property	Burglary	Larceny	Vehicle Theft
POST-1966	-.097 (-1.907)†	-.075 (-0.880)	-.133 (-2.394)*	-.150 (-1.762)†
VIOLENT CRIME RATE	.015 (0.611)	.023 (0.554)	-.015 (-0.544)	-.012 (-0.273)
OFFICERS (PER CAPITA)	-.196 (-2.009)†	-.089 (-0.556)	-.228 (-2.146)*	-.162 (-1.015)
OFFICERS/ NUMBER OF VIOLENT CRIMES	.313 (1.425)	.036 (0.101)	.748 (3.123)**	-.135 (-0.379)
POLICE EXPENDITURES (PER CAPITA)	.006 (1.771)†	-.002 (-0.375)	.010 (2.956)**	-.001 (-0.212)
POLICE EXPENDITURES/ NUMBER OF VIOLENT CRIMES	.002 (0.133)	.027 (1.150)	-.035 (-2.261)*	.032 (1.368)
CRIME IN SMALL CITIES[a]	-.003 (-1.396)	-.002 (-0.646)	-.002 (-0.919)	-.002 (-0.708)
TIME	-.004 (-0.495)	-.018 (-1.186)	.020 (2.097)*	-.021 (-1.396)
TIME^2	.00002 (0.209)	.0001 (0.504)	-.0003 (-2.074)*	.0003 (1.483)
INTERCEPT	3.027 (21.102)**	3.213 (14.148)**	2.853 (18.225)**	3.405 (14.932)**
Adjusted R^2	0.942	0.922	0.611	0.939

** significant at .01 level * significant at .05 level † significant at .10 level

Note: The dependent variable is the natural log of the reported clearance rate. These regressions are estimated using the Hildreth-Lu correction for serial correlation. All police and resource measures have been multiplied by the ratio of officers to total employees.

[a] This variable measures the percent of violent crimes committed in cities with fewer than 250,000 inhabitants.

APPENDIX

Variable Name	Description	Data Source	Explanation
Clearance Rates for Crimes (clrcitmu, clrcitra, clrcitrb, clrcitaa, clrcitbu, clrcitla, clrcitve)	Clearance rates for murder, rape, robbery, assault, burglary, larceny, and vehicle theft	FBI Uniform Crime Reports ("*UCR*")	This number comes from a subset of cities: 1950: 1601 cities, population 54,690,179 1995: 8278 cities, population 148,295,000. These population figures are termed "popclr."
Clearance Rates for Index Crimes (clrcitpc, clcitvc)	Clearance rates for property and violent crime	Prior to 1975, these numbers come from JAMES ALAN FOX, FORECASTING CRIME DATA: AN ECONOMIC ANALYSIS (1978). Fox's data are based on a weighted average of reported material from the FBI. For 1975-1995, these numbers come from the *UCR*	The FBI numbers come from a subset of the population: 1975: 6449 cities, population 127,068,000 1995: 8278 cities, population 148,295,000
Crime in Small Cities (smcity)	Percent of violent crimes committed in small cities (the total of all violent crimes committed in cities with population fewer than 250,000 inhabitants)	*UCR*	This number comes from a subset of cities: 1950: 2032 cities, population 41,529,235 1995: 7133 cities, population 100,029,000
Dollars Spent on Police Protection (dolcopin)	Dollars spent on police protection in 1995 dollars	Statistical Abstract	
Murder Rate (citmurat)	Murders per 100,000 each year in the cities reporting the clearance rates	This number was calculated by dividing the variable "citymurd" by "popclr" and multiplying by 100,000	
Number of Murders (citymurd)	The number of murders committed each year in the cities reporting the clearance rates	*UCR*	This number comes from a subset of cities: 1950: 1601 cities, population 54,690,179 1995: 8278 cities, population 148,295,000 Note: For 1958, 1959, and 1960 the number of murders was estimated since no data was available.
Number of Violent Crimes (vcrimes)	Total number of violent crimes in the U.S per year	Created by multiplying variable "vcrate" by "popres" (which is the U.S. population)	
Officers and Other Police Employees Per Capita (copfbirt)	Rate of total police employees per 1,000 people	*UCR*	This number comes from a subset of cities: 1950: 3223 cities, population 73,340,751 1995: 10,010 cities, population 164,313,000

Variable Name	Description	Data Source	Explanation
Officers Per Capita (offrte)	Rate of officers per 1,000 people	*UCR*	Before 1965, this number was created by multiplying the number of police employees, as reported in the *UCR*, by the percent that are civilian, as reported in the *UCR*. Once the number that are civilian is subtracted from the total number of employees, the resulting number (of officers) was transformed into a rate by dividing by the population of the cities that reported the number of employees. After 1965, the UCR begins to report the rate of officers so no calculation is necessary.
Officers/Murder (polcm)	The number of police per murder in cities reporting clearance rates	Created by dividing the variable "copfbirt" by "citmurat"	
Officers/Police Employees (ratoff)	Ratio of officer rate to total police employees reporting rate	Created by dividing variable "offrte" by the variable "copfbirt"	Both the denominator and the numerator come from a subset of cities that are identical prior to 1965 and different thereafter: 1950: 3223 cities, population 73,340,751 1995 ("copfbirt"): 10,010 cities, population 164,313,000 1995 ("offrte") 9970 cities, population 165,542,000
Officers/Violent Crime (polvc1)	Police officers per violent crime	Equals (copfbirt/vcrate)*ratoff	
Police Expenditures (dolrate1)	Police expenditures per capita	Equals (dolcopin/popres)*ratoff	
Police Expenditures/Murder (dolcm1)	Police dollars per number of murders in cities reporting clearance rates	Created by dividing the variable "dolcopin" (CPI-adjusted spending on police) by "citymurd"*ratoff	
Police Expenditures/ Violent Crime (dolvc1)	Police dollars per violent crime	Equals (dolcopin/vcrimes)*ratoff	
Population of the United States (popres)	The population of the U.S.	U.S. Bureau of the Census	
Population Reporting for the Clearance Rate (popclr)	The population of the cities reporting clearance rates	*UCR*	
Post-1966-1 (mira1)	Dummy variable = .5 in 1966 and 1 every year afterward; otherwise = 0		
Post-1966-2 (mira4)	Dummy variable = .33 in 1966, .66 in 1967, and 1 every year afterward; otherwise = 0		

Variable Name	Description	Data Source	Explanation
Time (timestep)	Years since 1949	Created a variable that equals the observation number (the data was already sorted chronologically)	
Violent Crime Rate (vcrate)	Violent crime rate	These numbers come from JAMES ALAN FOX, FORECASTING CRIME DATA: AN ECONOMIC ANALYSIS (1978). For 1975 and on they come from the *UCR*.	